

CHAPTER 1

INTRODUCTION

Background and Organization

In the last several years, a steady stream of reports has estimated that the rate of sea level rise is likely to accelerate in the next century (EPA 1983; NRC 1983; NRC 1985; IPCC 1990; Wigley & Raper 1992). As a result, coastal decisionmakers around the world have gradually begun to consider how to respond. In many cases, no immediate response is necessary, because the time required to implement a response is less than the time likely to pass before the sea rises significantly (NRC 1987).

A number of important decisions, however, are sensitive to sea level rise on time scales of a century or so. In some cases, the cost of preparing for a large rise in sea level is small compared with the costs that would eventually be incurred if the sea rises more than assumed in a project's design. In such a case, it is rational to design for a relatively high scenario, even if that scenario is unlikely. For example, the Dutch flood-protection system is designed to endure the "ten thousand year storm," which has only a 1 percent chance of occurring in a given century (Goemans 1986). Thus, if a new dike is expected to last a century, maintaining the desired level of safety requires an explicit consideration of the probability distribution of sea level rise.

Similarly, if a state intends to protect its coastal wetlands or the public's legal right to access along the shore, the cost of anticipatory land use planning can be less than 1 percent of the eventual cost of remedial action (Titus 1991); thus, it can be rational to implement these land use policies even for areas with a low probability of inundation. A few states have added restrictions to the development of coastal property which essentially say that if sea level rises enough to erode or inundate it, the property owner must remove any structures that impede the landward migration of natural shorelines.¹ If other states consider this option for protecting their tidelands, they may wish to determine the resulting impact on coastal property values.² Doing so requires an explicit assessment of the timing and likelihood of the sea rising enough to inundate a particular property.

¹E.g., South Carolina's Beachfront Management Act special permits; Texas' Open Beaches Act; and Maine's Dune Rule 355.

In spite of the need for this information, previous assessments of future sea level rise have not provided probabilities, for both computational and conceptual reasons. At the computational level, projections of sea level rise require complex nonlinear functions. Hence, even if we knew the distributions of the various uncertain processes, probability theory would offer us no direct "closed form" solution for estimating the probability distribution of future sea level rise. Instead, one must iteratively approximate the distribution by evaluating the models with alternate values for the various unknowns. But many models—particularly the "general circulation models" used to assess the impact of greenhouse gases on climate—cost too much to run for this to be possible.

Even where the computational problems can be solved, estimating probability distributions seems to involve more subjectivity.³ Existing measurements may lead researchers to be confident that a particular set of low, medium, and high scenarios are reasonable. But ascribing probabilities requires an additional level of specification, and current knowledge does not permit this to be done with precision. For example, both Meier (1990) and IPCC (1990) report the results of committees that agreed to a high scenario in which the Antarctic contribution to sea level rise is zero. The committees did not, however, decide whether "no Antarctic contribution" represents a worst-case scenario or a scenario with some chance of being exceeded. Had they decided upon the latter interpretation, they would have faced the additional difficulty of estimating the probability of such an exceedence, which would have required more subjectivity.

The main reason to estimate probability distributions is that decisionmakers need this information. If the published literature does not provide a proba-

²In some states, the common law allows the government to prohibit bulkheads; hence, allowing a bulkhead to be built provides a windfall to a riparian owner, the value of which the state may wish to consider. In other states, property owners have a right to build a bulkhead; a rule prohibiting bulkheads would decrease property values. In either case, a measure of the probability distribution is necessary to determine the present discounted value of the property being lost at some future date. See J.G. Titus (draft), "Rising Seas, Coastal Erosion, and the Takings Clause."

³In reality, the subjectivity is no greater. Whether one picks low and high values or ascribes a probability distribution, one must subjectively interpret the literature.

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bility distribution, then engineers, economists, and decisionmakers must implicitly or explicitly develop their own estimates, which are likely to be less accurate than the results of expert panels.⁴

This report presents the methods and results of a two-part effort to estimate the probability distribution of future sea level rise implied by the expectations of approximately twenty climate researchers. In the first phase, we developed a simplified model for estimating sea level rise as a function of thirty-five major uncertainties, derived probability distributions for each parameter from the existing literature, and conducted a Monte Carlo⁵ experiment using 10,000 simulations. The first portions of Chapters 2 through 6 summarize the model, distributions, and results of that “draft” analysis:

Chapter 2—emissions, concentrations, and atmospheric forcings of greenhouse gases;

Chapter 3A—the use of a 1-D ocean model for estimating global temperatures and sea level rise due to thermal expansion of ocean water; and simple relationships describing the dynamics of polar air and water temperatures as functions of global temperatures;

Chapter 3B—simple relationships describing changes in polar precipitation;

Chapter 4—the impact of warmer polar temperatures and precipitation changes on the contribution to sea level from the Greenland ice sheet;

Chapter 5—several alternative models relating polar warming to Antarctic ice discharges; and

Chapter 6—our adaptation of the IPCC model of the contribution to sea level from small glaciers.

⁴Focusing on probability distributions may also foster scientific cohesion by enabling scientific panels to avoid choosing sides in matters of scientific uncertainty, and instead lend partial credence to competing, contradictory viewpoints, until one or the other is disproved. For example, unlike previous EPA reports, this study does not reject out of hand the view of some “greenhouse skeptics” that greenhouse warming will be negligible. As discussed in Chapter 3, our simulations include the views of a representative skeptic.

⁵See Note 8, *infra*.

Figure 1-1 illustrates the relationships between the various models we used and developed to project sea level. Given the emissions projections, we used existing gas-cycle models to project atmospheric concentrations and the resulting radiative forcing (Chapter 2). We developed simple models of how upwelling may change, based on the results of three-dimensional models.⁶ We used an existing model to project the resulting temperature and thermal expansion estimates (Chapter 3). We devised simple models for projecting changes in polar climate and Antarctic water temperatures (Chapter 3), as well as the impact of water temperatures on ice-shelf melting (Chapter 5). We developed a simple model of a possible fast-but-stable impact of ice-shelf melting on the Antarctic ice sheet contribution, while using existing models to simulate an unstable response and a stable-but-slow response (Chapter 5). We developed a simple model of how the runoff elevation in Greenland responds to climate change, but used existing models to project the actual contribution of the Greenland ice sheet to sea level (Chapter 4). We used an existing model to estimate the impact of small glaciers on sea level (Chapter 6). To estimate relative sea level at a specific location, one can combine tidal-gauge observations with the estimated glacial and thermal expansion contributions (Chapter 9).

In the second phase of this study, we circulated the draft report to a “Delphic” panel of experts⁷—approximately two dozen climatologists and glaciologists, listed in Table 1-1. In each case, we directed their attention to specific chapters, and asked them to review our assumptions, and suggest the assumptions that they would have used had they conducted the analysis. A few of the researchers provided comments without probability distributions; but twenty of the researchers did give us their best assessment of the values of the model coefficients most closely related to their own research. Moreover, five researchers even provided alternative model specifications. Given the probability distributions specified by our Delphic panel of experts, we reran the 10,000 simulations.

⁶Additional models were added in the second phase, based on the expert reviews.

⁷Broadly defined, a Delphic assessment is an analysis based in part on the opinions of experts. The origin of the term stems from the oracles at Delphi in Greek mythology, who, among other things, warned Oedipus that he would kill his father; they were also known as oracles of Apollo, the god of prophesy. The expert opinions of a Delphic assessment, like the pronouncements of the oracles at Delphi, are presumed valid regardless of whether there is an explanation supporting them. Nevertheless, in this report, the reviewers generally do provide explanations.

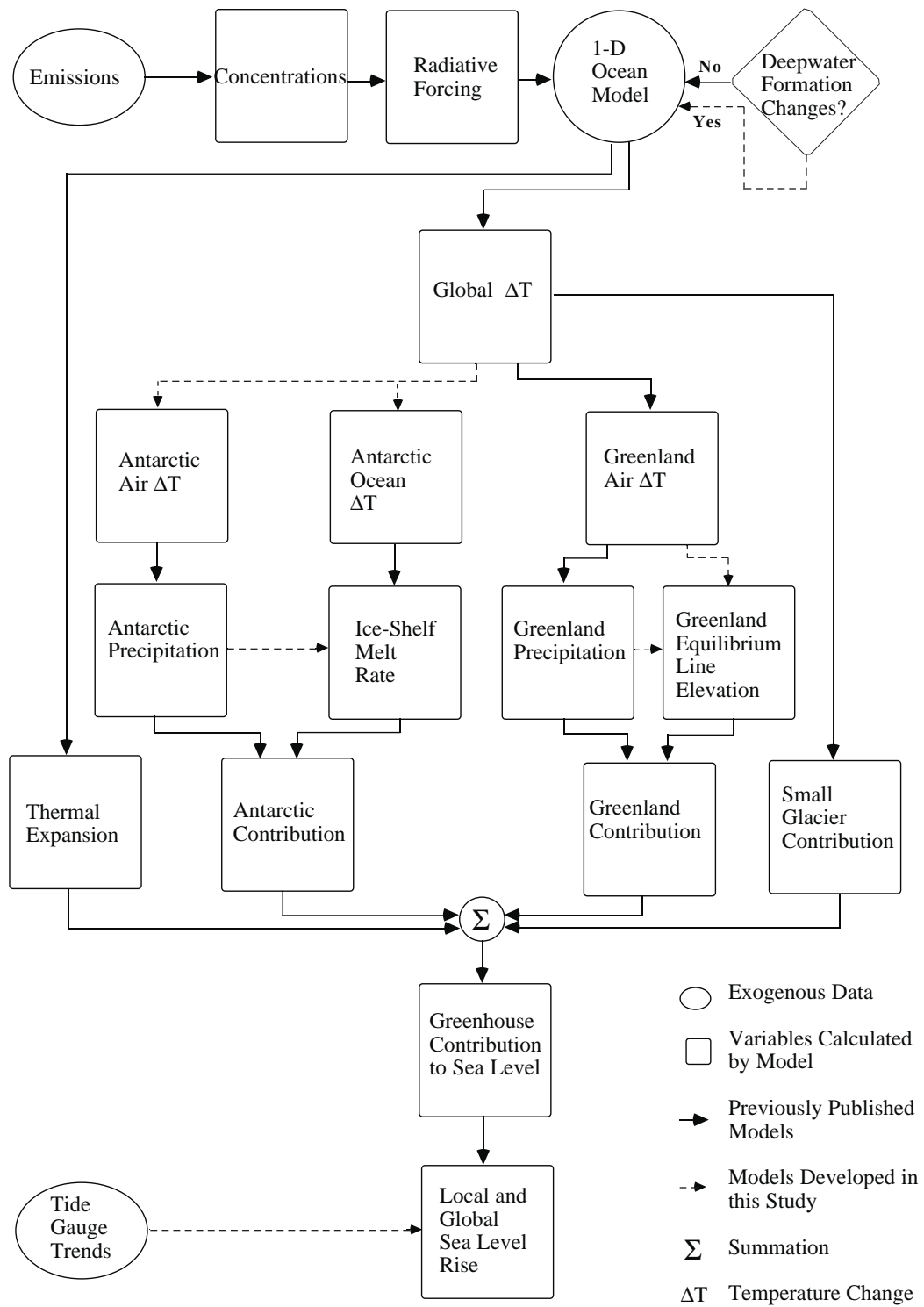


Figure 1-1. Relationship Between the Various Models We Used to Project Sea Level.

TABLE 1-1
 REVIEWERS WHO CONTRIBUTED TO THIS ANALYSIS

Global Climate and Polar Temperature Assumptions

Robert Balling	Arizona State University	Tempe, AZ
Francis Bretherton	University of Wisconsin	Madison, WI
Martin Hoffert	New York University	New York, NY
Michael MacCracken	Lawrence Livermore National Laboratory	Livermore, CA
Syukuro Manabe	NOAA/Princeton Geophysical Fluid Dynamics Laboratory	Princeton, NJ
David Rind	NASA/Goddard Institute for Space Studies	New York, NY
Stephen Schneider	Stanford University	Stanford, CA
Sarah Raper ^a	University of East Anglia	Norwich, UK
Tom Wigley ^a	University Corporation for Atmospheric Research	Boulder, CO

Polar Precipitation Assumptions

Richard Alley	Pennsylvania State University	Univ. Park, PA
Michael Kuhn	Innsbruck University	Innsbruck, Austria
Michael MacCracken	Lawrence Livermore Nat. Laboratory	Livermore, CA
David Rind	NASA/Goddard Institute for Space Studies	New York, NY
Stephen Schneider	Stanford University	Stanford, CA
Jay Zwally	NASA/Goddard Space Flight Center	Greenbelt, MD

Antarctic Assumptions

Richard Alley	Pennsylvania State University	Univ. Park, PA
Anonymous	University Professor	United States
Charles Bentley	University of Wisconsin	Madison, WI
Robert Bindshadler	NASA/Goddard Space Flight Center	Greenbelt, MD
Stan Jacobs	Lamont Doherty/Columbia University	Palisades, NY
Craig Lingle	University of Alaska	Fairbanks, AK
Robert Thomas	NASA/Greenland Ice Core Project	Washington, DC
C.J. van der Veen	Ohio State University	Columbus, OH
T. Wigley and S. Raper ^a	University of East Anglia	Norwich, UK
Jay Zwally	NASA/Goddard Space Flight Center	Greenbelt, MD

Greenland Reviewers^b

Walter Ambach	University of Innsbruck	Innsbruck, Austria
Robert Bindshadler	NASA/Goddard Space Flight Center	Greenbelt, MD
Roger Braithwaite	Geological Survey of Greenland	Copenhagen, Dmk
Mark Meier	University of Colorado	Boulder, CO
Robert Thomas	NASA/Greenland Ice Core Project	Washington, DC
T. Wigley and S. Raper ^a	University of East Anglia	Norwich, UK
Jay Zwally	NASA/Goddard Space Flight Center	Greenbelt, MD

^aWigley and Raper provided a joint review based on their revisions to an unpublished analysis initiated by Richard Warrick. The Wigley & Raper study is summarized in Wigley, T.M.L., and P.D. Jones. 1992. "Detection of Greenhouse Gas Induced Climatic Change." Research Proposal to U.S. Department of Energy. During the study, Wigley moved from East Anglia to University Corporation for Atmospheric Research.

^bThe Greenland reviewers offered modeling suggestions but did not suggest independent parameter values, except for Wigley & Raper.

In the latter part of each of the following chapters, we summarize the reviewer changes and present the results of the Delphic Monte Carlo experiment. We discuss the draft and Delphic assumptions separately for two reasons. First, the separate discussion helps to avoid ambiguity with regard to which assumptions were developed by us and which were provided by the reviewers. Second, and perhaps more importantly, in many cases particular reviewers decided that the parameters from the draft were reasonable enough. For example, based on the commonly accepted 1.5 to 4.5°C warming from a CO₂ doubling, we assumed that the most likely value is 2.6°C, which is the geometric mean of this range. All but one of the researchers accepted this characterization. Had we used the arithmetic mean of 3.0°C, most of the reviewers may well have accepted that formulation as well. Here and elsewhere, our initial specifications almost certainly had a lingering effect on the results of the analysis. By discussing the draft and the Delphic analysis separately, we enable readers to (a) examine how the reviewers changed our assumptions and (b) thereby evaluate the extent to which our initial assumptions may have biased the analysis.

The last three chapters present our final results. Chapter 7 summarizes the results of our analysis, focusing on the likely impact of greenhouse gases on temperatures and global sea level, and examining the sensitivity of the results to alternative emissions scenarios and other assumptions. Chapter 8 places the results in context, examining both the reasons that sea level projections have been revised downward and the practical uses to which sea level projections have been put. Finally, Chapter 9 explains how to use our estimates to project local sea level at specific locations.

How Much of This Report Is Worth Reading?

We warn the reader at the outset that, for all but a limited audience, most of this report is exceedingly dry—particularly Chapters 3, 4, and 5. The typical coastal engineer, geologist, lawyer, or policy analyst may prefer to read only Chapters 7, 8, and 9. For the more technical reader who is already familiar with the assumptions underlying the IPCC and other sea level rise assessments, it may be sufficient to read the sections entitled “**Expert Judgment**,” particularly in Chapters 3A, 3B, and 5, along with the results reported in Chapter 7. Those trying to understand how this analysis differs

from previous assessments should focus on the remainder of this Chapter and the “**Expert Judgment**” section in Chapter 3.

The remainder of this chapter summarizes methodological issues that are relevant to all of the chapters.

Approach

Our overall approach is to assume that

$$SL = M(a, b, c, \dots),$$

where SL is sea level,
M is the model, and
a, b, c, ... are unknown coefficients.

We assume that the model would be true if we knew the actual values of the coefficients. But because no one knows their precise values, we must rely on estimates, each of which is uncertain. Based on available estimates and reasonable assumptions about the shapes of the distributions, one can estimate a probability density function for each coefficient.

In the simple case, where $SL = aX + bY$ and we have data on X and Y, probability theory provides us with a simple formula for estimating the distribution of SL. Projections of sea level rise, however, are nonlinear: Even simple models must multiply uncertain temperatures by uncertain melting-sensitivity parameters, and most models are far more complex. Under these circumstances, solving for the distribution is too complicated to be practical.

Statisticians have shown, however, that one can eventually converge on the distribution by randomly selecting values of the coefficients, running the model repeatedly, and treating the resulting estimates as a sample. This procedure is known as “Monte Carlo.”⁸ Because we wanted to estimate the rise with

⁸The meaning of the term “Monte Carlo analysis” has evolved. Originally, the term referred to the use of many trials to numerically approximate a probability distribution—as opposed to analytically solving the equations. As the use of Monte Carlo techniques evolved, mathematicians have shown that the original approach of randomly selecting the input values is not as efficient as nonrandom sampling approaches such as Latin Hypercube. Although Latin Hypercube is a Monte Carlo technique in the original sense of the word, many authors use the term “Monte Carlo analysis” to refer only to exercises that employ totally random samples.

a 1 percent chance of being exceeded, 10,000 trials seemed to be sufficient.⁹

Table 1-2 lists thirty-five parameters used by the draft report. In most cases, we characterized probability distributions derived from the literature. In four cases, however, the draft used alternative models; in these cases, we specified n -nomial distributions based on our best guess about the combined opinion of the community.¹⁰ For example, if we have two alternative models for estimating thermal expansion of ocean water, we assume that there is a chance of p that $SL=M_1(a,b,...)$ and a chance of $(1-p)$ that $SL=M_2(a,b,...)$. Although this approach allows us to relax the assumption that a particular model is true, it still understates our uncertainty because there is a chance that none of the models we specify are either true or reasonably accurate summaries of the likely response of the relevant processes.

Combining Reviewer Opinions. Once the reviewers had reacted to our original draft by providing us with their subjective probability distributions, we had to decide (a) how to ensure that the insights of one reviewer would feed back onto the opinions of the other reviewers, and (b) how to combine the reviewer opinions to develop a probability distribution that fairly incorporates the combined wisdom of all the reviewers. Because of time and cost limitations, we followed the simplest approach that we could devise. Our feedback process primarily involved (1) circulating each of the reviewer assessments to all of the reviewers of a particular chapter; (2) notifying each reviewer if another reviewer questioned any aspect of his or her assessment; and (3) giving each reviewer an opportunity to change his or her subjective probability distributions based on the assessments of the other reviewers. We also played “Devil’s Advocate” with each reviewer. For each para-

meter, we would discuss the potential implications of the reviewer’s specified distribution to ensure that the reviewer was providing a well-considered opinion.

Our final estimates reported in Chapters 7 and 9 are based on weighting each opinion equally. We concede at the outset that there are more sophisticated ways for combining reviewer opinions. For example, we might have polled a second, independent group of experts regarding the validity of the opinions of the first group of experts, or we might have polled the original group regarding the credibility of other reviewers on specific parameters.¹¹ Because such iterations were not feasible,¹² however, weighting the opinions equally seemed justified under the circumstances.¹³ The reviewers who participated represent a fair cross-section of scientific opinion regarding the key areas of climate sensitivity, polar temperature, polar precipitation, and glacier sensitivity.

Recognizing that other researchers may wish to weight the reviewer opinions differently,¹⁴ we report all of the recommended probability distributions of every reviewer. So that the reader of this report can

⁹The random Monte Carlo approach is not as efficient at estimating the extremes of a cumulative distribution as the Latin Hypercube method, but the complex weighting required by that algorithm would have required considerable time to implement. Moreover, Latin Hypercube might not have been very effective in our case unless we ran millions of trials. Unless the parameters are uncorrelated, Latin Hypercube requires many more trials than we conducted before its superiority emerges. As discussed below, there are thirty-five parameters, with complex functional relationships between many of them (see *Correlations Between Parameters*, *infra*). Even if there were only eight parameters, with distributions divided into four segments for sampling, the sample space would have 4^8 (i.e., 65,536) different areas that had to be sampled; assuming that each required at least ten observations, one would require 650,000 simulations. See **Numerical Error of the Monte Carlo Algorithm**, Chapter 7, *infra*.

¹⁰This approach was extended in the final version, in two ways. First, several reviewers provided additional models from which to select. Second, our approach for incorporating the reviewer comments essentially treated each reviewer’s opinion as a separate model from which to select.

¹¹To call these more iterative methods a “Delphi” approach is somewhat of a misnomer: the oracles at Delphi did not provide commentary on the validity of the pronouncements of other oracles. Nevertheless, these iterative approaches are generally referred to as “Delphi.”

¹²So that other researchers might use this report for other purposes, we wanted to keep this analysis “on the record,” which would have been impossible if the reviewers had to rate the expertise of other scientists. A few reviewers had indicated at the outset that they would participate only if each opinion was counted equally. Moreover, as we interviewed most of the other researchers, we got the distinct impression that putting probabilities on scientific processes that they had studied was already a novelty, and that asking them to weight the opinions of other reviewers was beyond what they wanted to do. (Two reviewers did, however, indicate that they would have preferred to participate in a second iteration concerning the relative expertise of the various reviewers.)

¹³Additional iterations would probably have been more important were it not for the fact that obtaining the reviewer opinions was already a second iteration for this study, the initial iteration being the draft report we circulated, which was based on parameters obtained from the literature.

¹⁴Theoreticians of decision analysis generally disapprove of the practice of weighting all opinions equally. Nevertheless, Winkler (1971) and Seaver (1978) “have found little or no difference in the performance of various differential weighting schemes over equal weighting....” (Morgan & Henrion (1990) at 167).

A more complex weighting scheme is possible only if there is a group of experts ready and willing to assess the validity of the original set of subjective probability distributions. If the political or monetary cost of independently evaluating the experts is high relative to the cost of obtaining the opinions in the first place, there may not even be a theoretical justification for the more complex weighting schemes. See e.g., Morgan & Henrion at 167 (“The administrator of EPA, or his surrogate, is likely to have difficulty publicly stating that he finds Dr. Jones’s views six times more credible than Dr. Smith’s views....”).

TABLE 1-2
INITIAL ASSUMPTIONS IN DRAFT REPORT

(Also used to represent some runs in the final report, where reviewer did not suggest changes)

Parameter	Parameter Name	Distribution Shape, Moments	Value of Moments	Correlation with Other Parameters
CONCENTRATIONS OF GREENHOUSE GASES				
Emissions	E	Nordhaus & Yohe, scaled	IPCC92 scenarios for each gas	perfect correlation
OCEAN MODEL PARAMETERS				
Equilibrium ΔT_{2XCO_2}	ΔT_{2X}	lognormal, σ limits	1.5, 4.5 °C	none
Diffusivity	k	lognormal, 2σ limits	1000, 3000 m ² /yr	w (1.0)
Probability of Case A	C1	binomial	Prob(C1 = 1) = 0.5	none
<u>Case A: Fixed Bottomwater Formation</u>				
Downwelling Ratio	π	lognormal, 2σ limits	0.2, 1.0	none
Upwelling Velocity	w	lognormal, 2σ limits	2.0, 6.0 m/yr	k (1.0)
<u>Case B: Bottomwater Formation Declines with Temperature</u>				
Downwelling Ratio	π	Fixed	0.2	none
Upwelling Velocity Initial	w_0	lognormal, 2σ limits	2.0, 6.0 m/yr	k (1.0)
Transient	w	$w(\Delta T) = w_0 \theta^{\Delta T}$		See function
Sensitivity of w to Temperature	θ	lognormal, 2σ limits	0.85 ² , 1.0	none
POLAR CLIMATE				
<u>Equilibrium Polar Amplification</u>				
Antarctic Summer	P1	lognormal, σ limits	0.67, 1.5	P2 (0.5), P3
Antarctic Winter	P2	lognormal, σ limits	1.0, 3.0	P1 (0.5)
Greenland Annual	P7	lognormal, 2σ limits	1.0, 2.0	P1, P2 (0.5)
Circumpolar Ocean	P3	lognormal, σ limits	0.25, 1.0	P1 (0.75)
<u>Adjustment Times (in addition to the global lag)</u>				
Circumpolar Ocean	P4	lognormal, 2σ limits	20, 80 years	P5, P6 (0.5)
Antarctic Summer	P5	lognormal, σ limits	1, 20	P6, P4 (0.5)
Antarctic Winter	P6	lognormal, σ limits	1, 20	P4 (0.5)
Greenland	—	Fixed	No Additional Lag	

TABLE 1-2 (continued)

Polar Precipitation

Antarctic	P8	lognormal, 2σ limits	See Table 3-3 (approx. 6%/°C)	P8 (0.5)
Greenland	P9	lognormal, 2σ limits	$V(t)/V(0)$, (9% $\Delta T = 1$) $V'(t)/V'(0)$ (8.5%)	P7 (0.5)
Antarctic Precip. Adjustment for Area	P10	lognormal, 2σ limits	1/3, 2/3	none

ANTARCTIC ICE SHEET AND ICE SHELF ASSUMPTIONSIce Shelf Melt

Seaice Sensitivity to Global Temperature	P10	lognormal, 2σ limits	0.05, 0.2	$P10 = e_{w/T}$
Sensitivity of Ross Ice Shelf Warm Intrusions	1+A1	lognormal, 2σ limits	1, 36	none
Ross Melt Response to Warm Intrusion	A2	lognormal, 2σ limits	0.25, 1.0	none
Probability of Undiluted CDW Under Ross	C3	binomial	$\min(0.05\Delta T_{cdw}, 0.25)$	none
Sensitivity of Weddell Sea to T_{cdw}	A3	fixed	1.0	none
Ronne/Filchner Basal Melt from Weddel Warming	A4	lognormal 2σ limits	1.91, 3.33	none
Threshold for Melt Only Model	A7	Right Triangular	$p(x) = 2x$ $F(x) = x^2$	none
Ice Stream Model				
Initial Velocity of Ice Stream B	V0	lognormal, 2σ limits	100, 300 m/yr	none
Upstream Length, Shelf Backpressure	L	lognormal, 2σ limits	100, 300 km	none
Calving	C2	Trinomial Fixed Calving Reference Calving Enhanced Calving	$P(C2 = 2) = 0.7$ $P(C2 = 0) = 0.3$ $P(C2 = 1) = 0.0$	none

NOTE: $V(t)$ is the saturation vapor pressure at a particular time. $V'(t)$ is dV/dT at a particular time. e is elasticity.

TABLE 1-2 (continued)

ANTARCTIC ICE SHEET MODEL SELECTION

<u>Model</u>	<u>Probability (%)</u>
AM1, IPCC No Ice Sheet Response, Precipitation Only	10
AM2, Basal Melt Only	20
Thomas Ice Stream—Extrapolation Options	
AM3, Continent Wide	5
AM4, Only to Streams that flow Through Shelves	10
AM5, Ratio of Ice Discharge to Melting	10
AM6, Ice Stream Specific Response	25
AM7, Oerlemans Model—Linearization	20

GREENLAND

Zero Ablation Line Response to ΔT	G1	lognormal, σ limits	111.1, 186.3 m/°C	none
Calving Response to Ablation	G2	normal, 2σ limits	0, 1.14	none
Response Time Due to Refreezing	G3	lognormal, σ limits	12.5, 50 years	none

SMALL GLACIERS

Response Time	τ	lognormal, σ limits	10, 30 years	none
Historic Contribution				
Oerlemans	M1	normal, σ limits	0.515, 1.885 cm	none
Meier	M2	normal, σ limits	1.2, 4.4	none
Probability of the Meier Estimate	C4	binomial	$P(C4=1) = 0.5$	none

gain a rough understanding of the results implied by each reviewer's assessments, we also disaggregate results by reviewer, where feasible. For example, for each climate reviewer (Chapter 3A), we report global and Greenland temperature estimates, as well as the Greenland, Antarctic, and total sea level contribution.¹⁵ *Because of the procedures we followed, our final results must be viewed as conditional probability estimates—conditional on the assumption that the participating*

¹⁵The estimates of sea level contribution by climate reviewer, however, require assumptions regarding glacier parameters, for which the climate reviewers generally expressed no opinion. For these assumptions, we weight all nonclimatic reviewers equally. (The Wigley & Raper assessment was an exception to this procedure, as explained below.)

reviewers adequately represent the cross-section of scientific knowledge on the parameters for which they provided probability distributions.

Correlations Between Parameters. For a variety of reasons, our uncertainty regarding one parameter may be related to our uncertainty regarding another parameter. As discussed in Chapter 3, for example, the parameters **k** (diffusivity) and **w** (upwelling velocity) used in ocean models are often viewed as being perfectly correlated, because the pattern by which ocean water temperatures decline with increasing depth is consistent with the assumption that $k/w=500$ meters.¹⁶

¹⁶See Chapter 3 for additional discussion of these parameters.

At least some of the factors that might lead Antarctic winter temperatures to warm could also cause summer temperatures to warm (*e.g.*, the latitudinal ocean circulation); so there is some correlation between summer and winter warming, albeit less than perfect. The draft accounted for some of these relationships by generating random values of the parameters with specified correlations.

The various reviewers of Chapter 3 suggested several additional correlations. For example, because reduced thermohaline circulation¹⁷ might imply a weaker Gulf Stream with which to heat Greenland, one researcher had a correlation of 0.5 between possible changes in \mathbf{w} and Greenland temperatures. Another reviewer assumed that the warming of the Antarctic circumpolar ocean will lag farther behind global temperatures in cases where emissions grow more rapidly or the climate sensitivity parameter ΔT_{2X} is larger; again a correlation of 0.5 was used.

The Delphic Monte Carlo analysis includes a second type of correlation, designed to preserve the internally consistent visions of the future implied by particular reviewers' assumptions. For example, although most reviewers of Chapter 3 did not specify a correlation between π and changes in \mathbf{w} , there was a tendency for those who expected a low π to also expect a decline in \mathbf{w} , and for those who used high values of π to consider \mathbf{w} as less likely to decline. We preserve the "consistent visions" by generating separate probability distributions for each researcher, rather than by developing a single composite distribution for each parameter.

For the most part, these consistent visions apply only to a particular chapter. The joint review provided by Tom Wigley and Sarah Raper, however, provided assumptions sufficient to estimate all of the contributors to sea level. Therefore, we treat their consistent vision as applying to the entire analysis; simulations representing their suggestions on warming, for example, are not combined with anyone else's assumptions regarding Antarctica.

¹⁷Thermohaline circulation refers to ocean currents driven by different densities, which in turn result from different temperatures and salinities. For example, evaporation over the Gulf Stream increases the salinity level and thereby the density of ocean water, enabling water to sink as it reaches the North Atlantic, forming deep water. This sinking helps propel the circulation that causes the Gulf Stream to flow north. Some climatologists expect warmer global temperatures to cause more rainfall over the North Atlantic, which would reduce salinity and deepwater formation, and thereby slow the Gulf Stream.

Our procedure for preserving these correlations is analogous to treating the reviews of each chapter as a deck of cards. Separate groups of reviewers provided comments on the nonprecipitation climate variables (Chapter 3A), precipitation (Chapter 3B), Greenland (Chapter 4), and Antarctica (Chapter 5). Our procedure was as follows:

1. We divided the assumptions into six decks:

Deck 2: This deck has 10,000 cards, each of which has a random value for each parameter discussed in Chapter 2.

Deck 6: Same as Deck 2, for Chapter 6.

Deck 3A: This deck is composed of eight piles, each of which corresponds to one expert reviewer, with the first pile representing Wigley & Raper. Each pile has 1250 cards, each of which has a random value for each of the nonprecipitation climate parameters discussed in Chapter 3. Each pile uses different underlying distributions corresponding to the distributions suggested by the particular researcher.

Deck 5: Same as Deck 3A, for Chapter 5.

Deck 3B: Same as Deck 3A, except that only six researchers provided distributions, so there are only six piles.

Deck 4: Same as Deck 3A, except that seven of the eight piles are drawn from the same underlying distribution. The first pile represents the distributions specified by Wigley & Raper. The remaining seven piles are drawn from the distributions accepted by the glaciologists who reviewed Chapter 4.

2. The top pile in each deck represents the suggestions of Wigley & Raper, because their joint review was the only review that suggested parameters for the whole array of sea level contributors. We remove the top pile from each stack and set it aside temporarily.

3. We shuffle the remaining piles of Decks 3B and 5. If we did not shuffle Deck 5, for example, the simulations that use the suggestions of the last reviewer of Chapter 3A would only use the parameters specified by the last reviewer of Chapter 5. By shuffling the deck, the simulations using this last climate reviewer use the assumptions of all the Antarctic (Chapter 5) reviewers in roughly equal proportions. There is no need to shuffle Deck 2 or 6, because they are already randomly mixed, as are the remaining seven piles of Chapter 4.
4. We put the Wigley & Raper piles back on the top of each deck.
5. We draw the top card from each deck and run a simulation using the parameter values. We then draw the next card from each deck and repeat the process for all 10,000 simulations.

Thus, the first 1250 simulations represent the consistent vision of Wigley & Raper across all chapters. The following 1250 simulations use the consistent vision of the second climate reviewer but include a random selection of parameters drawn from all other chapters.

Time Horizon. Like most previous assessments of sea level rise, we focus on the year 2100. However, we do not truncate our analysis at that date. We extend our analysis farther into the future for both technical and policy reasons.

On the technical side, several glacial modeling efforts have suggested that impacts from Antarctica will not be significant until after the year 2100 (e.g., Huybrechts & Oerlemans 1990). Yet the potential impacts have long been discussed. To end our analysis before Antarctica is likely to have a significant impact, would lead our assessment to exclude consideration of some of the most important research on the issue of long-term sea level rise. If we could be certain that Antarctica will not make a contribution within the relevant time horizon, disregarding that research might be warranted; however, no such certainty exists. In a similar vein, examining longer time horizons helps to provide a better understanding of the implications of one's assumptions, and the impacts likely to occur over longer periods of time are similar to the worst-case scenarios of what could happen in the next century.

On the policy side, no one has demonstrated that impacts after the year 2100 are irrelevant. The remoteness of the twenty-second century, we suggest, can be better addressed by discounting the future than by ignoring it completely. Policymakers concerned with nuclear waste sites have considered potential consequences thousands of years into the future. The roads that are built today can determine the locations of development for centuries into the future, even if specific structures only last one-hundred years. Although local planning commissions generally focus on the next few decades, the civic groups that propose policies often include churches and historic preservation groups with perspectives stretching back several centuries. Finally, Cline (1992) argues that all climate impact assessments should extend two-hundred years into the future, and at least one chapter of a draft IPCC report has attempted to extend the analysis out several centuries (Pearce et al. 1994).

Most officials will be more concerned with "best-guess" estimates for the next few decades. But the importance or lack of importance of very-long-run and very-low-probability impacts can only be ascertained if impact analysts have scenarios of these remote contingencies.

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